<u>РЕЦЕНЗИЯ №1</u>

<u>на статью</u> **«COMPARATIVE ANALYSIS OF MULTIDECADAL VARIABILITY OF HYDROMETEOROLOGICAL PARAMETERS IN THE PONTO-CASPIAN SEAS»**
<u>авторского коллектива</u>**: Kazmin A. S., Shiganova T. A.**

Этап №1

This paper considers multidecadal variability in the time series of the annual means of temperature, wind as well as characteristics of humidity and precipitation in the regions of the Black, Azov and Caspian Seas using polynomial approximations of long-term variability.

Generally, it is an interesting study which shows the association between atmospheric dynamics and thermal and humidity characteristics in theses regions. However, I have several methodological and editorial comments listed bellow to be addressed and I suggest a major revision of this manuscript prior it is considered for publication.

<u>Major Comments:</u>

1. This paper was submitted in “Journal of Oceanological Research” but I don’t see any oceanographic context except for the selected regions which located over seas. Also, in the introduction the authors argue that atmospheric conditions may influence the marine ecosystems and potential reader would expect that this paper concentrates on this link besides conventional description of variability of the atmospheric parameters.

However, in the paper this issue is not covered at all. In lines 150–152 it is pointed that *“data on invasive ctenophores… were obtained from….”* but I could not see were and how these data were used throughout the paper. Moreover, the title of the paper is also not reflecting to full extent the contents of the paper. The word “abiotic” while formally acceptable here, implicitly points to the biological context of the manuscript which is not detectable, be it implicitly or explicitly. “*Abiotic*” would be better changed to “atmospheric”; in this case the title will be more consistent with what this paper is about. While in this case the submission to the ocean research journal still remains a bit of a problem (at least for me), this can be left for the editorial option. One solution could be to add a section about the relation between atmospheric variability and the invasions of ctenophores.

2. The choice of the research areas. It is a priori clear that meteorological conditions are very different in these two regions (in spite of their relatively close location to each other) because of the local orography and well know circulation conditions in these regions and this is fully supported by this study. The authors should provide a better justification why they expect some interrelation between these two regions. Argumentation which was given in the first paragraph and going along the arguments that in the Pliocene period these two marine domains were consolidated in a single water reservoir is not enough.

3. Polynomial approximation. It is a very reasonable argument (posed in the MS) that over the long-time atmospheric parameters are not changing linearly and that linear trends may not reflect well the observed changes. However, you should better justify the choice of the order of degree of the approximation because it is significantly influence on the periodicity of changes. Also, this approximation could be not optimal for different variables. If polynomial approximation for temperature looks very reasonable (fig.2), for moisture parameters it does not look as the best solution (fig.6).

4. Data. Authors used low resolution NCEP reanalysis with spatial resolution of about 2.5 degree. For relatively small regions as Black and Caspian Seas which are known by specific local atmospheric conditions such a coarse spatial resolution does not allow for capturing the regional features well. NCEP reanalysis is a spectral model output at resolution of T62 for derivatives and T42 for many surface variables. Try to plot spectral orography of NCEP-1 (it is available from NCEP domain) to see how your region looks like in T62 spectral resolution.

5. Seasonality is also important issue here. Authors argue that for the temperature winter and summer differences are not too important but question comes, how it is for the other variables such as wind, humidity and precipitation?

6. Normalization. When you normalize your variables with the parameters of Gaussian distribution you implicitly imply that all of them are distributed normally. This is true for daily and monthly temperatures but it is not obvious for the annual values. On the other hand, wind speed, precipitation and humidity characteristics are not following Gaussian distribution with wind speed being likely distributed according to Weibull PDF and precipitation following gamma distribution (while other options are available as well, but not Gaussian). Thus, the use of the parameters of Gaussian distribution for normalization of all variables should be checked and justified.

7. Correlation. The procedure of choosing of points selected for the correlation analysis should be described better. How do you choose the time intervals which represent decrease or increase of the temperature? Are they based upon the locations of black triangles in fig. 7a, b or what? If so, why the periods in fig 8 are different from the time intervals imposed by the locations of triangles? For example, fig.8a shows correlation for the period of decreasing temperature for 1948-1964 but in fig.7a a minimum is sticked to 1972. The same for the other periods. Also, what is exactly shown by black triangles in fig.7? By glance I see that for example location of triangle in panel b is not matching the maximum. The same is visible for panels d and f. Also how do you place the blue rectangles in this figure?

8. *Lines 363-365*. Explanation of the dependency of precipitation on the cooling and warming mediated by the increase or decrease of the relative humidity is not correct. Relation between RH and precipitation is strongly nonlinear and much more complicated that one is assumed here, this is also demonstrated in fig.7e.

9. Section about the impact of NAO and EAWR as it stands now is not very informative and is unlikely useful. I don’t see in fig.10 any visible relation between zonal wind component and NAO and EAWR. If this section remains in the paper, it should be enriched by the quantitative estimates.

<u>Minor comments:</u>

1. Section titles. Section 2 has a title “Materials and methods”. I would suggest “Data and methods”. Section 3 is currently “data description” but you already described your data. Data description is usually the description of data themselves as a source of data, resolution, some problems with this data etc. I would suggest to us something like ‘data preprocessing’. Subsection 3.2 is entitled “polynomial approximation”. But you have already presented this approximation in the previous subsection and this subtitle doesn’t add much. This subtitle can be deleted and the subsection can be merged with the previous one.
2. Introduction should be partly re-written to reflect better the contents of the paper (see my comment 1 above). If you don’t investigate the influence to the ecosystem, you can just mention general importance of this. Now it seems not to be the main focus of this paper.
3. The word “dramatically” (*line 13*) should be used with care; there are general caveats against this word in scientific literature. You can replace it with “significantly” or smith similar.
4. Figs 8 and 9. Looks (by eye) that the plotted regression lines correspond to the one-side regressions, not to orthogonal ones. Try to check this. This does not affect the correlations (as they are scaled with STDs, but changes the angle and potentially intercept.

<u>Подпись. Рецензент №1. 11.10.2022.</u>

От редакции: рецензия была направлена редакцией авторскому коллективу.

<u>Ответ рецензенту №1</u> на Рецензию от 11.10.2022 г. на статью авторского коллектива: Kazmin A. S., Shiganova T. A. «COMPARATIVE ANALYSIS OF

MULTIDECADAL VARIABILITY OF HYDROMETEOROLOGICAL PARAMETERS IN THE PONTO-CASPIAN SEAS».

Reviewer: *This paper considers multidecadal variability in the time series of the annual means of temperature, wind as well as characteristics of humidity and precipitation in the regions of the Black, Azov and Caspian Seas using polynomial approximations of long-term variability.*

Generally, it is an interesting study which shows the association between atmospheric dynamics and thermal and humidity characteristics in theses regions. However, I have several methodological and editorial comments listed bellow to be addressed and I suggest a major revision of this manuscript prior it is considered for publication.

Major Comments:

1. This paper was submitted in "Journal of Oceanological Research" but I don't see any oceanographic context except for the selected regions which located over seas.

Author: IMHO, processes over oceanic/marine basins **do** belong to "oceanology" scope. And it is a multidisciplinary journal anyway

Reviewer: *Also, in the introduction the authors argue that atmospheric conditions may influence the marine ecosystems and potential reader would expect that this paper concentrates on this link besides conventional description of variability of the atmospheric parameters. However, in the paper this issue is not covered at all. In lines 150-152 it is pointed that "data on invasive ctenophores… were obtained from…." but I could not see were and how these data were used throughout the paper.*

Author: it is our fault that we did not clean the text completely (originally the paper supposed to be submitted to another journal). Now all inconsistencies are deleted throughout the text.

Reviewer: *Moreover, the title of the paper is also not reflecting to full extent the contents of the paper. The word "abiotic" while formally acceptable here, implicitly points to the biological context of the manuscript which is not detectable, be it implicitly or explicitly. "Abiotic" would be better changed to "atmospheric"; in this case the title will be more consistent with what this paper is about.*

Author: in the title "ABIOTIC" changed to "HYDROMETEOROLOGICAL".

Reviewer: *While in this case the submission to the ocean research journal still remains a bit of a problem (at least for me), this can be left for the editorial option. One solution could be to add a section about the relation between atmospheric variability and the invasions of ctenophores.*

Author: this stuff has been deleted, please see above.

Reviewer: *2. The choice of the research areas. It is a priori clear that meteorological conditions are very different in these two regions (in spite of their relatively close location to each other) because of the local orography and well know circulation conditions in these regions and this is fully supported by this study. The authors should provide a better justification why they expect some interrelation between these two regions.*

Author: no any "interrelation" is expected or declared (this word has been deleted, *line 103*); just an opposite, we stressed the pronounced differences of multidecadal variations in two areas.

Reviewer: *Argumentation which was given in the first paragraph and going along the arguments that in the Pliocene period these two marine domains were consolidated in a single water reservoir is not enough.*

Author: there is no any **argumentation** at all, just very brief historical reference.

Reviewer: *3. Polynomial approximation. It is a very reasonable argument (posed in the MS) that over the long-time atmospheric parameters are not changing linearly and that linear trends may not reflect well the observed changes. However, you should better justify the choice of the order of degree of the approximation because it is significantly influence on the periodicity of changes. Also, this approximation could be not optimal for different variables. If polynomial approximation for temperature looks very reasonable (fig.2), for moisture parameters it does not look as the best solution (fig.6).*

Author: degree of polynomial approximations was selected to highlight the gross features of multidecadal variability. It is different for each time series. A proper phrase is added (*line124*).

Reviewer: *4. Data. Authors used low resolution NCEP reanalysis with spatial resolution of about 2.5 degree. For relatively small regions as Black and Caspian Seas which are known by specific local atmospheric conditions such a coarse spatial resolution does not allow for capturing the regional features well. NCEP reanalysis is a spectral* *model output at resolution of T62 for derivatives and T42 for many surface variables. Try to plot spectral orography of NCEP-1 (it is available from NCEP domain) to see how your region looks like in T62 spectral resolution.*

Author: we are not into capturing **local** features. Our goal was to highlight the gross features of multidecadal variability on annual and basin-wide scales (as stated at *line 139*). As you can clearly understand, switch to another dataset means new full-scale study. Probably, it worth to perform this in the future.

Reviewer: *5. Seasonality is also important issue here. Authors argue that for the temperature winter and summer differences are not too important but question comes, how it is for the other variables such as wind, humidity and precipitation?*

Author: similar results have been obtained for the wind and moisture content of the atmosphere. Graphs are not presented so as not to overload the manuscript. A proper note added to the text (*lines 140-141*).

Reviewer: *6. Normalization. When you normalize your variables with the parameters of Gaussian distribution you implicitly imply that all of them are distributed normally. This is true for daily and monthly temperatures but it is not obvious for the annual values. On the other hand, wind speed, precipitation and humidity characteristics are not following Gaussian distribution with wind speed being likely distributed according to Weibull PDF and precipitation following gamma distribution (while other options are available as well, but not Gaussian). Thus, the use of the parameters of Gaussian distribution for normalization of all variables should be checked and justified.*

Author: these requirements seem too excessive for an article positioned as descriptive-geographical (*line 10, lines 102-103*).

Reviewer: *7. Correlation. The procedure of choosing of points selected for the correlation analysis should be described better. How do you choose the time intervals which represent decrease or increase of the temperature? Are they based upon the locations of black triangles in fig. 7a, b or what? If so, why the periods in fig 8 are different from the time intervals imposed by the locations of triangles? For example, fig.8a shows correlation for the period of decreasing temperature for 1948-1964 but in fig.7a a minimum is sticked to 1972. The same for the other periods. Also what is exactly shown by black triangles in fig.7? By glance I see that for example location of triangle in panel b is not matching the maximum. The same is visible for panels d and f. Also how do you place the blue rectangles in this figure?*

Author: Figs. 8 and 9 supposed to illustrate in a simple and clear way correlations between wind components and between wind components and air temperature. The time intervals are chosen in such a way as to demonstrate this as clearly as possible. Black triangles at the abscissa axis of Fig. 7 mark the years of highs/lows of the corresponding parameters or, if you prefer, a mode change. Since this is not an instantaneous process, their position rather conditionally marks some average moment of transition while the transition itself lasts for several years. The same is for colored rectangles: they are chosen rather visually to qualitatively show the periods of parameters increase/decrease.

Reviewer: *8. Lines 363-365. Explanation of the dependency of precipitation on the cooling and warming mediated by the increase or decrease of the relative humidity is not correct. Relation between RH and precipitation is strongly nonlinear and much more complicated that one is assumed here, this is also demonstrated in fig.7e.*

Author: sorry, but this is about "precipitable water" and its connection with temperature, not about "precipitation". It is cautiously declared that "no statistically significant correlation between variations of precipitable water and air temperature was found" (*line 369-370*).

Reviewer: *9. Section about the impact of NAO and EAWR as it stands now is not very informative and is unlikely useful. I don't see in fig.10 any visible relation between zonal wind component and NAO and EAWR. If this section remains in the paper, it should be enriched by the quantitative estimates.*

Author: as mentioned (*line378-379*), only some debatable assumptions, based on qualitative phenomenological analysis have been presented. For the Caspian we see the reasonable consistence of NAO/EAWR variability and zonal wind. As for the Black Sea, the situation (as stated) is controversial. Still, we believe that this illustration, while qualitative, is useful for general insight and understanding of the processes.

Reviewer: *Minor comments:*
1. Section titles. Section 2 has a title "Materials and methods". I would suggest "Data and methods".
Author: changed (line 108).

Reviewer: *Section 3 is currently "data description" but you already described your data. Data description is usually the description of data themselves as a source of data, resolution, some problems with this data etc. I would suggest to us something like 'data preprocessing'. Subsection 3.2 is entitled "polynomial approximation". But you have already presented this approximation in the previous subsection and this subtitle doesn't add much. This subtitle can be deleted and the subsection can be merged with the previous one.*
Author: We met all those suggestions, thank you!

Reviewer: *2. Introduction should be partly re-written to reflect better the contents of the paper (see my comment 1 above). If you don't investigate the influence to the ecosystem you can just mention general importance of this. Now it seems not to be the main focus of this paper.*
Author: all this have been done (please see response to comment 1 above).

Reviewer: *3. The word "dramatically" (line 13) should be used with care; there are general caveats against this word in scientific literature. You can replace it with "significantly" or smith similar.*
Author: corrected, replaced with "significantly".

Reviewer: *4. Figs 8 and 9. Looks (by eye) that the plotted regression lines correspond to the one-side regressions, not to orthogonal ones. Try to check this. This does not affect the correlations (as they are scaled with STDs, but changes the angle and potentially intercept.*
Author: the point is to demonstrate statistically significant correlations between wind and temperature. The angle and intercept are not discussed. The only phrase mentioning "slope" (lines 322-324) is deleted.

The authors are grateful to the respected reviewer for careful acquaintance with the manuscript and useful comments, most of which we met with gratitude and corrected. A small part of the requirements seems too excessive, taking into account the general context of the work and it's positioning ("descriptive-geographical") and is left to the discretion of the Editor.

С уважением, авторский коллектив. 24.10.2022.

От редакции: ответ и доработанная версия статьи были направлены редакцией рецензенту.

Этап №2

From the revised version I realize that the authors provided some substantial changes in the manuscript according to my comments. However, few points remain still questionable and need to be addressed:

1. I still cannot see enough argumentation for considering the Black sea and the Caspian Sea in a whole. This is not the major issue, of course, but for me it would be useful to add a clear statement about the reasoning for doing this. Again, for now thing in whole is a [relatively] close geographical location of these basins, but other basins can be mentioned in this respect as well (e.g. Red Sea which is also not far away). 2. I still do not clearly understand you answer about low spatial resolution of NCEP-1 data and normalization of humidity, precipitation and wind with parameters of the Gaussian distribution.

Generally, your argumentation going along the statement that the article is "positioned as descriptive-geographical" one is not to a full extent acceptable for scientific paper. Also it is unclear for me what do you mean under "to highlight the gross features" when you consider the local (relatively small) basins? It is quite obvious that the parameters of normal distribution could not be used for the normalization of variables which are distributed abnormally (that should be tested by the way). Even if you decline to redo the computations you can at least put a caveat on this point in the manuscript. Thus, I leave these points for the editor's decision. 3. I copy here my comment 8 and the author's answer: 8. Lines 363-365. Explanation of the dependency of precipitation on the cooling and warming mediated by the increase or decrease of the relative humidity is not correct. Relation between RH and precipitation is strongly nonlinear and much more complicated that one is assumed here, this is also demonstrated in fig.7e.

Author: sorry, but this is about "precipitable water" and its connection with temperature, not about "precipitation". It is cautiously declared that "no statistically significant correlation between variations of precipitable water and air temperature was found" (line 369-370).

From the text (lines 374-376) we get "The simplest qualitative explanation for the observed dependence of precipitation rate on temperature may be that during periods of cooling/warming, relative humidity increases/decreases, as shown above." Thus, it is not about precipitable water but rather about precipitation rate. Consequently, argumentation given here is not totally correct and has to be changed or simply removed. Again, "Descriptive-geographical" status of this paper is not a good excuse for the wrong statements. Try to edit this place.

Подпись. Рецензент №1. 10.11.2022.

От редакции: повторная рецензия была направлена редакцией авторскому коллективу.

Ответ рецензенту №1 на повторную Рецензию от 10.11.2022 г. на статью авторского коллектива: Kazmin A. S., Shiganova T. A. «COMPARATIVE ANALYSIS OF MULTIDECADAL VARIABILITY OF HYDROMETEOROLOGICAL PARAMETERS IN THE PONTO-CASPIAN SEAS».

Reviewer: *From the revised version I realize that the authors provided some substantial changes in the manuscript according to my comments. However, few points remain still questionable and need to be addressed:*

1. I still cannot see enough argumentation for considering the Black sea and the Caspian Sea in a whole. This is not the major issue, of course, but for me it would be useful to add a clear statement about the reasoning for doing this. Again, for now thing in whole is a [relatively] close geographical location of these basins, but other basins can be mentioned in this respect as well (e.g. Red Sea which is also not far away).

Author: the reason is that Black and Caspian seas belong to so called Ponto-Caspian basin with common geological history, similar salinity conditions (brackish) and located in the same climate zone. Since after construction of the Volga-Don Canal the seas were artificially reunited again, both native and invasive species from the Black Sea enter and established (due to similar environmental conditions) in the Caspian Sea, shaping its ecosystem. That is why it is important to compare the variability of abiotic parameters in these seas together. Introduction has been edited (line 37-46). (Regarding e.g., Red Sea – it is tropical basin with very high salinity and temperature and in no way suitable for comparison).

Reviewer: 2. *I still do not clearly understand you answer about low spatial resolution of NCEP-1 data and normalization of humidity, precipitation and wind with parameters of the Gaussian distribution.*

Generally, your argumentation going along the statement that the article is "positioned as descriptive-geographical" one is not to a full extent acceptable for scientific paper. Also it is unclear for me what do you mean under "to highlight the gross features" when you consider the local (relatively small) basins? It is quite obvious that the parameters of normal distribution could not be used for the normalization of variables which are distributed abnormally (that should be tested by the way). Even if you decline to redo the computations you can at least put a caveat on this point in the manuscript. Thus, I leave these points for the editor's decision.

Author: I agree to leave it to the discretion of the Editor Under "gross features" I mean variability on yearly – basin wide scales, in contrast to small-scale variability (weeks/months – tens of kilometers).

Reviewer: *3. I copy here my comment 8 and the author's answer: 8. Lines 363-365. Explanation of the dependency of precipitation on the cooling and warming mediated by the increase or decrease of the relative humidity is not correct. Relation between RH and precipitation is strongly nonlinear and much more complicated that one is assumed here, this is also demonstrated in fig.7e.*

Author: sorry, but this is about “precipitable water” and its connection with temperature, not.

С уважением, авторский коллектив. 24.10.2022.

От редакции: ответ и доработанная версия статьи были направлены редакцией рецензенту.

Подтверждение Рецензента №1 на публикацию:

Здравствуйте. Я посмотрела ответ и статью. Она может быть опубликована с точностью до реакции редактора на 2-е замечание во второй рецензии.

Это важный вопрос, но я оставляю решение по этому вопросу за редактором.

Подпись. Рецензент №1. 14.11.2022.

От редакции: аспекты статьи, касающиеся второго замечания рецензента, были рассмотрены и утверждены на публикацию научным редактором «Океанологических исследований».